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ATTRIBUTION THEORY AND JUDGMENT UNDER UNCERTAINTY

Baruch Fischhoff

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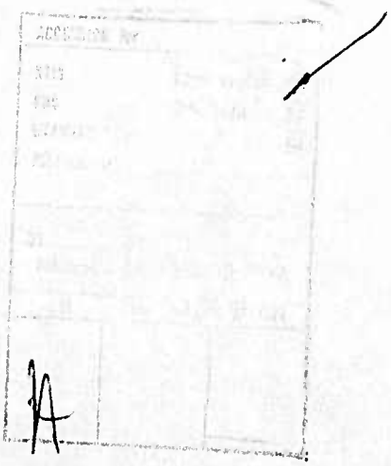

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13. ABSTRACT

Research in two areas of human information processing, attribution theory and judgment under conditions of uncertainty, is characterized and compared. Differences in the picture of people's inferential ability which emerge from the two areas are highlighted and attributed, in part, to researchers' paradigmatic conventions. Ways to reduce the divergence of these two fundamentally complementary lines of investigation are suggested.

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Attribution Theory and Judgment under Uncertainty

Baruch Fischhoff

Oregon Research Institute

Running Head: Attribution Theory and Judgment under Uncertainty

Attribution Theory and Judgment under Uncertainty

Two of the most active areas of research into inferential behavior are the approaches generally known as "judgment and decision making under conditions of uncertainty" (here, "judgment") and "attribution theory." The former primarily deals with predictive inferences about unknown events--typically set in the future. The latter deals with how people attribute causes to or explain events which have already transpired.

Formally, the two areas differ in their respective subject matter: prediction and explanation. More striking, however, is the difference in the picture of men and women which emerges from them. Attribution researchers find people to be effective processors of information who organize their world in a systematic manner prone to relatively few biases. Judgment research reveals people to be quite inept at all but the simplest inferential tasks--and sometimes even at them--muddling through a world that seems to let them get through life by gratuitously allowing for a lot of error.

To illustrate this contrast, in a central article in attribution theory, Kelley (1973) likens man to an intuitive scientist; in a central article in judgment research, Slovic and Lichtenstein (1971) seriously question the notion of "man as an intuitive statistician." Whereas it has been recommended (Kelley, 1972b, p. 171) that future attribution research make greater and more explicit assumptions about people's causal sophistication, judgments researchers have often gone in quite the opposite direction, looking for ever more biases in people's judgments and for ways in which fallible people can be wholly or partially removed from their own decision-making processes.

Why do these divergent images emerge from research in these two areas? One possible explanation is that people are excellent explainers, but poor predictors. What empirical evidence there is, however, seems to indicate that people may be even worse at explanation than prediction. Indeed, this evidence, albeit collected in the judgment tradition, suggests that people's problems with prediction are due in part to the poor quality of their explanations (Fischhoff, 1974, 1975).

The explanation of this contrast will be sought rather in the paradigmatic properties of the two research areas. Both judgment and attribution research have many of the characteristics of full-blown research paradigms: pet problems, unquestioned assumptions, recognized centers of research activity. In contrast with Kuhn's (1962) conception of the paradigm, however, the two areas are not completely incommensurable. As we shall see, many of the incapacities which each approach has built into its activists are due more to investigators' fairly arbitrary conventions and interests than to their allegiance to metatheoretical assumptions. Thus, contrasting the two has, hopefully, a good chance of generating some light as well as heat.

Ideally, such a discussion should begin with either definitive statements of judgment research and attribution theory or at least a summary of relevant research. As the former is presently unavailable--owing to the youth and diversity of the two areas--and the second beyond the scope of this paper, I begin instead with brief descriptions of several studies which seem to typify work done in each area (Section 1). Section 2 considers results emerging in each area which are directly relevant to the

other. Section 3 considers several paradigmatic assumptions that emerge from analysis of the studies cited, assumptions that seem to be the source of the divergence of judgment and attribution research. Section 4 suggests directions for future research and cross-fertilization.

1. Characteristic Research

(a) Judgment.

Three focal topics of judgment research have been the ways in which people (1) make subjective probability estimates, (2) sequentially update such estimates upon receipt of additional information, and (3) simultaneously combine probabilistic information from multiple sources. In each area subjects' performance has been compared with a normative criterion of judgmental adequacy and found lacking. With many probabilistic tasks, people appear neither to produce the responses demanded by these normative models nor to process information in ways indicated by the models.

Probability estimates. Perhaps the simplest task involving probabilistic inference is that used in probability learning experiments. In a typical study of this type subjects might be asked to predict the color of each of a series of marbles drawn from an urn containing an unspecified mixture of red and blue marbles. In reviewing these studies, Vlek (1970; also Luce & Suppes, 1965; Peterson & Beach, 1967) noted that although subjects are able to accurately estimate the proportion of marbles of each type, they do not use this information effectively. Rather than consistently predicting the more frequent type of marble, a strategy that would maximize their accuracy, subjects typically predict the less

frequent color in a substantial proportion of trials. Provision of payoffs weakens but does not eliminate the biase (e.g., Messick & Rapoport, 1965). The patterns of subjects' predictions appear to reflect complex, idiosyncratic theories about sequential dependencies in random series, theories which have no basis in the mathematical theory of binomial processes. "Gamblers' fallacy" is a related example of people's misconceptions of how random sequences should look.

When frequentistic data, such as the proportion of red and blue marbles, is lacking, it is difficult, if not impossible, to assess the veracity of any individual probability estimate. If someone assigns a probability of .80 to there being a Democrat in the White House in 1984, there is no way now, nor will there be a way in 1985, to tell how good that estimate was. It is, however, possible to evaluate the validity of a set of probability estimates. The measure of their validity is their degree of calibration. For perfectly calibrated judges, XX% of the events to which they assign .XX probability of occurrence will, in fact, occur. In empirical tests, calibration is typically quite remiss. Fischhoff and Beyth (1975), for example, found underestimation of low probabilities and overestimation of high ones. Such predictions meet too many big surprises, very unlikely events that occur and very likely ones that do not. Other investigators (e.g., Attneave, 1953; Cohen & Christensen, 1970) have found just the opposite, overestimation of low probabilities and underestimation of high ones. No comprehensive theory of miscalibration is currently available.

When asked to quantify their confidence about knowing the correct answer to general knowledge questions (i.e., estimate the probability that their chosen answer is correct), subjects typically overestimate how much

they know. For example, Alpert and Raiffa (Note 1) had subjects set upper and lower limits for possible values of quantities like the population of Outer Mongolia so that there was but a 2% chance that the true answer fell outside of the limits. Across problems, some 40% of the true values fell outside of the confidence intervals. People also have been found to exaggerate their ability to predict entities like horse races (Scott, 1968), the stock market (Fama, 1965) and natural hazards (Kates, 1962).

Perhaps even more disturbing is the fact that people's probability estimates frequently violate the most basic laws of probability theory. For example, Kahneman and Tversky (1973), Hammerton (1974), and Lyon and Slovic (1975) have found that when people are called upon to combine base rate information with evidence regarding a specific case, they consistently ignore the base rate information, even when the "individuating evidence" has negligible validity. Years ago, Meehl and Rosen (1955) found a similar problem to affect the developers of psychometric tests.

Tversky and Kahneman (1971; 1974) found that people are virtually oblivious to questions of sample size, exaggerating the stability of results obtained from small samples and failing to see the increased stability to be found in larger samples. In their earlier article (Tversky & Kahneman, 1971), this bias was dubbed "belief in the law of small numbers."

In a number of studies, Wyer (1974; Wyer & Goldberg, 1972) has found that people consistently overestimate the likelihood of the conjunction of two events. Slovic, Fischhoff and Lichtenstein (1976) have shown that the judged probability of compound events may actually be larger than the probability of their constituent events. The probabilities assigned to an exhaustive set of mutually exclusive events have frequently been found not to sum to 100, another violation of internal consistency in people's intuitive

judgments (see Peterson & Beach, 1967, p. 36). Summarizing his work on the interrelations between people's probabilistic beliefs, McGuire (1968) concluded that they do not have the sort of internal coherence demanded by the laws of probability.

Opinion revision. One crucial aspect of functioning in a probabilistic environment is being able to update properly one's beliefs about that environment upon receipt of additional information. The consensual normative model for opinion revision is Bayes' Theorem, whose implications are fully explicated in Edwards, Lindman, and Savage (1963), Phillips (1973), and Slovic and Lichtenstein (1971).

An extensive research program has investigated people's intuitive adherence to Bayesian inference. Whereas initial work appeared to indicate that people were generally quite sensitive to the parameters of the Bayesian model (e.g., Peterson & Beach, 1967), more recent reviews have been considerably more pessimistic. Slovic and Lichtenstein (1971, p. 714) conclude,

> ...the intuitive statistician appears to be quite confused by the conceptual demands of probabilistic inference tasks. He seems capable of little more than revising his response in the right direction upon receipt of a new item of information (and the inertia effect is evidence that he is not always successful in doing even this). After that, the success he obtains may be purely a matter of coincidence--a fortuitous interaction between the optimal strategy and whatever simple rule he arrives at in his groping attempts to ease cognitive strain and to pull a number "out of the air."

The simple rules which people appear to use (e.g., Dale, 1968; Kahneman & Tversky, 1972; Lichtenstein & Feeney, 1968; Pitz, Downing, & Reinhold, 1967) not only fail to produce accurate Bayesian estimates, but they also have no analog in the formal model. Thus the model fails both to predict subjects' responses and to capture the essential determinants of their judgment processes.

Information integration. One crucial skill for anyone living in an uncertain world is the ability to combine information from a variety of sources into a single diagnostic or prognostic judgment. Such tasks constitute the life work of stock analysts, investigative radiologists, and major league scouts; they confront all of us daily in enterprises as diverse as forming impressions and deciding when to cross the street.

Perhaps the best-known work on how well people integrate information from multiple sources is the research on clinical psychologists' judgmental processes summarized and inspired by Meehl's (1954) Clinical versus statistical prediction. These studies, reviewed more recently by Goldberg (1968; 1970), indicate that rather simple actuarial formulae typically can be constructed to perform at a level of validity no lower than that of the clinical expert.

This disturbing finding produced a great deal of research into why clinical judges did no better, research that in turn produced even more disturbing findings. For example, the accuracy of judges' inferences appears to be unrelated to either the amount of information in their possession or their level of professional training and experience (see Goldberg, 1968, and references therein).

Other findings indicate that the "rules" of intuitive information integration are fundamentally inconsistent with normative principles of optimal information utilization. Kahneman and Tversky (1973) found that when making inferences on a given set of cues, people are more confident when they believe that those cues are redundant than when they believe the cues to be independent. Normatively, in such a situation, redundant cues carry less information and, thus, justify less confident judgments. Tversky (1969) found that subjects' preferences between multi-attribute alternatives are sometimes intransitive. In a numberical prediction task requiring the utilization of but two cues, Lichtenstein, Earle, and Slovic

(1975) discovered that subjects used a non-normative averaging heuristic which coincidentally provided reasonable predictions. Slovic and MacPhillamy (1974) found that when called upon to choose between multi-attribute alternatives, people are unreasonably influenced by commensurable dimensions, those that can be readily compared across possible choices. An example might be choosing the cheapest of several alternative summer vacation plans, not because cost is of utmost importance, but because it provides the one dimension on which all possibilities can be unambiguously characterized and compared.

Although recent work (e.g., Dawes and Corrigan, 1974) suggests that the superiority of actuarial predictions may say more about the power of the models than about the impotence of their human competition, these other disturbing results remain on the record.[1]

Interpretation. One central notion in explaining these biases is that of "cognitive strain." In many tasks judges are confronted with more information than they are able to process. To cope with this overload, they develop ad hoc algorithms, or heuristics, for information processing. In this view, people are seen as computers that have the right programs but frequently cannot execute them properly because their central processor is too small.

Some of these biases, for example the insensitivity to sample size or the preference for redundant information, illustrate more serious deficiencies. Here, people's probabilistic judgments are not only biased or incomplete, but fundamentally wrong. Returning to the computer analogy, it appears that people lack the correct programs for many important judgmental tasks. Even more disturbing is the fact that neither decisions made by experts nor decisions with grave social consequences are immune to these biases (Goldberg, 1968; Slovic, 1972; Slovic, Fischhoff, & Lichtenstein, 1976; Slovic, Kunreuther, & White, 1974; Tversky & Kahneman, 1971).

(b) Attribution.

The central concern of attribution research is people's intuitive

perceptions of causality, specifically, their attributions of reasons for the occurrence of behavioral events. Although attribution research is quite diverse, much of it can be traced to the work of Heider (1958), as developed and operationalized by Jones and Davis (1965), Kelley (1967, 1972a, 1972b, 1973), Weiner (1974) and others. As several recent reviews are readily available (e.g., Ajzen & Fishbein, 1975; Miller & Ross, 1975; Shaver, 1975), this section will be even more abbreviated than the previous one.

Kelley's analyses provide perhaps the most general framework for studying attributional processes.[2] In them he distinguishes between situations in which historical data regarding the behavior in question are available and situations in which they are not. In the former situations he hypothesizes three characteristics of the behavior to be explained that will govern an observer's atributions. They are the behavioral act's consistency over time (does the actor always respond that way?), its distinctiveness (is it elicited by other stumulus situations as well?) and its degree of consensus (do other actors respond similarly?). These historical data are seen as being organized in a three-dimensional matrix from which attributions are derived in keeping with J. S. Mill's method of difference. Kelley translates this method into the "covariation principle," according to which acts are attributed to possible causes with which they covary. From this hypothesis, he derives many interesting predictions which have been most thoroughly examined by MacArthur (1972).

MacArthur systematically varied consensus, distinctiveness, and consistency information pertaining to a given behavioral act (e.g., John laughs at the comedian). Subjects were told "Your task is to decide on the basis of the information given, what probably caused the event to occur. You will be asked to choose between four alternative causes . . . the cause which you think is most probable." The alternatives were (a) something about the

person (e.g., John), (b) something about the stimulus (e.g., the comedian), (c) something about the particular circumstances, (d) some combination of a, b, and c. Two main findings were (1) that each of the three sorts of information affected attributions to some degree; and (2) that there was a preponderance of person attributions.

This latter finding has since been replicated by Jones and Nisbett (1972) and others who have found that the same act of behavior will often be attributed internally (to the actor) by an observer but externally (to the stimulus circumstances) by the actor himself. These "divergent perceptions" of the causes of behavior have been explained as due to differences in the information available to the observer and actor regarding the act in question (see also Weiner and Seirad, 1975).

When historical data are lacking, Kelley (1972b, 1973) sees people relying on what he calls "causal schemata." In his words (Kelley, 1972b),

> a causal schema is an assumed pattern of data in a complete analysis of variance framework. What the person learns at a conceptual level . . . is how certain types of effects tend to be distributed in a matrix of relevant causes. Given information about a certain effect and two or more possible causes, the individual tends to assimilate it to a specific assumed analysis of variance pattern and from that to make a causal attribution. (p. 152; italics in original)

These schemata might be thought of as general types of laws of behavior. For example, one principle of behavior which many people appear to accept is that in order to succeed on a difficult task one must both be capable and try hard (Kun & Weiner, 1973). This principle is an example of a multiple necessary causal schema, the sort of rule which, according to Kelley (1972b), is invoked to account for unusual occurrences (see also Enzel, Hansen, & Lowe, 1975), or events of great magnitude (Cunningham & Kelley, 1975).

In trying to account for the way in which people infer underlying dispositions from observation of behavioral acts, Jones & Davis (1965) hypothesized that the observer does the following: (1) identifies the choice options facing the actor; (2) lists the actor's possible reasons for selecting each act; (3) eliminates those reasons that could have motivated the selection of acts other than the one chosen; and (4) assesses the importance for the actor of each of the remaining reasons. The basis of this last assessment is the perceived importance of each reason (the desirability of the anticipated effect) for members of the various reference groups to which the actor belongs. If the observer can identify a reason that could only motivate selection of the chosen act and that is not highly valued by others, then he or she will attribute the act to that reason and the underlying personal disposition that it represents. "We can be certain that a politician who advocates achieving cuts in government spending by lowering social security payments to an audience of senior citizens <u>really</u> means what he says" (Shaver, 1975, p. 48).

<u>Specific topics</u>. The attribution paradigm has been used to advance understanding in a number of substantive areas. Weiner and his associates (1972) have convincingly shown how people's attributions of the causes of success and failure can both influence and be influenced by their motivations. Practitioners of "attribution therapy" (Valins & Nisbett, 1972; Loftis & Ross, 1974) have helped clients to tolerate emotionally upsetting stimuli by inducing them to attribute their aroused state to some emotionally irrelevant or controllable source. Jones and Goethals (1972) have looked at order effects in impression formation from an attributional perspective. Feldman-Summers and Kiesler (1974) have used an attributional task to elicit aspects of sex role stereotypes, finding that subjects who have observed identical performance by male and female actors attribute greater motivation to the women.

Much of this work has focused on questions of social perception, presumably because attribution theory has grown within the context of social psychology. In many ways the paradigm seems to have encouraged and legitimized asking a variety of new and illuminating questions about behavior. It has also somewhat restructured the role of the social psychologist which Kelley (1973) believes "is not to confound common sense, but rather to analyze, refine, and enlarge it" (p. 172).

(c) Contrast.

The similarities between judgment and attribution research must be apparent from even these brief reviews. Both study how people interpret, organize, and use multivariate information in an uncertain environment. Both are largely phenomenological in their theorizing, attempting to understand in common sense terms the ways in which people think about their world. There are even some vague similarities between the theories developed in each. For example, Kahneman and Tversky's (1972) representativeness heuristic, which leads a judge to view a possible event as likely if it embodies or "represents" the main features of the situation or person creating it, seems related to Jones and Davis' (1965) notion that an attributed personal disposition can be seen as characterizing the overt action from which it is inferred.

The image of people's information-processing ability that emerges from these two areas is, however, strikingly different. In judgment research people seem to do so poorly that cataloguing and shoring up their inadequacies has become the focal topic of research. In attribution research, they either are found to do quite well or the question of adequacy never comes up.

These generalizations are not without exceptions. Peterson and Beach (1967) did identify a number of tasks, primarily making intuitive estimates of descriptive statistics, that people performed quite well. A continuing

goal of current research has been to find out what tasks people do well on, what tasks they might do better on with training, and what tasks are best taken out of their hands entirely and allocated to machines and actuarial formulae. Students of biases have also indirectly acknowledged people's inferential abilities by the great ingenuity they have shown in trying to elicit clearly biased behavior.

Similarly, there has been study of "attributional biases." In particular, investigators have looked at ways in which attributors distort incoming data to better serve their own ego-defensive functions or their sense of control over their world (e.g., Caldini, Braver, & Lewis, 1974; Kelley, 1967, 1972a; Luginbuhl, Crow, & Kahan, 1975). The discrepancy in attributions for one's own behavior and that of others has also been discussed as reflecting a bias toward inferring personalogical traits where there are none--a bias that is shared by psychologists and laymen alike (e.g., Jones & Nisbett, 1972; Mischel, 1968). Kanouse (1972) has presented some evidence indicating that people are prone to primacy effects (i.e., relying on the first sufficient explanation that comes to mind) in their attributions. Walster (1966; 1967) and others (e.g., Vidmar & Crinklaw, 1974) have looked at people's defensive attribution of responsibility for accidents and their tendency to exaggerate the predictability of accidents that threaten them--and, thus, their ability to avoid the danger.

However, not only is the study of biases somewhat the exception in attribution work, but the robustness of even these biases has recently been seriously questioned by attribution researchers (Ajzen & Fishbein, 1975; Miller & Ross, 1975; Ross, Bierbrauer & Polly, 1974; Taylor, 1975). In addition, it will be noted that in these examples it is not the attributional information processing that is being questioned but the information that the attributor

uses. Attributional biases are essentially proper conclusions drawn from improper premises. The impropriety of the premises arises either from inefficient information gathering or hedonic distortion of what is happening, not from difficulties in handling or combining information. Judging from the literature, attribution researchers appear to assume that (1) people use the attributional techniques which they (the psychologists) hypothesize; (2) they use them properly; and (3) these techniques provide adequate guides for making attributions. Returning to the computer analogy, the naive attributor is seen as having both the proper programs and the capacity to execute them.

In Kelley (1972b), for example, this viewpoint emerges not only from his presentation of the schemata concept but also in his discussions of its limitations (pp. 171-3)--which are essentially ways in which the theory underestimates people's attributional sophistication. So much faith is placed in people's inferential abilities that attributional theories are often produced by first discerning (often with great ingenuity) the information to which people will attend in a given situation and then formally working through the conclusions which may be properly deduced from them.

2. Mutually Relevant Studies

Although attribution and judgment research have generally gone on their separate courses with few glances to the side, there are a number of studies of mutual interest. Several, such as the work in Bayesian opinion revision and on the inconsistency in personal belief systems have been presented above. Others appear below. Most of these, it seems, suggest ways in which judgmental biases may intrude on attributional tasks.

(a) Use of base rate information.

Perhaps the most direct and creative integration of judgment and attribution work is a recent study by Nisbett and Borgida (Note 2). After reviewing studies testing Kelley's covariance model, they conclude that although there is much evidence showing people's sensitivity to consistency and distinctiveness information, there is little evidence of a similar sensitivity to consensus information. They note that this failure to use information about what most people do in a particular situation is directly analogous to Kahneman and Tversky's (1973) finding that people ignore base rate information in favor of individuating information about the case at hand. In an ingenious study, they replicate this finding using behavioral base rate information, providing a judgmental reason for the failure of Kelley's model in this respect.

It is unclear how these results can be reconciled with the heavy reliance on base rate or reference group data in a wide variety of situations postulated by Jones and Davis (1965). One possibility is that the norms of the reference group are considered only insofar as they are embedded in the description of the actor. They than become some of the actor's characteristics which should be "represented" (in Kahneman and Tversky's, 1972, sense) in any behavioral act. In that case, they would be considered more for their associational value than their informational worth.

Nisbett and Borgida suggest that "perhaps in fact, it is only when we have rather well-rehearsed schemata for dealing with certain types of abstract, data-summary information that it is used in a fashion that the scientist would describe as rational" (p. 26). What evidence there is of subjects' responding to consensus information (Frieze & Weiner, 1971; Weiner & Kukla, 1970) comes, indeed, from attributions for success and failure, situations for which "well-rehearsed schemata" do seem to be available (Weiner, (1974).

(b) Defensive attribution and hindsight.

A reverse confluence of research efforts emerged in some of my own work (Fischhoff, 1974, 1975). As mentioned above, Walster (1967) found that when confronted with news of an unfortunate accident, people tend to exaggerate in retrospect its predictability. I found this result to be a special case of a more general phenomenon. In general, events which are reported to have occurred are seen in hindsight as having appeared more likely (and thus predictable) in foresight than they actually did appear. I argued that by exaggerating the predictability of the past, people underestimate what they have to learn from it.

A corollary of this bias is that in hindsight we find it very difficult to reconstruct the uncertainties which faced other decision makers in the past. In second-guessing others we typically overestimate the clarity with which they foresaw what was going to happen (Fischhoff, 1974; 1975, Experiment 3). These results suggest a further source of bias in dispositional attributions produced by users of Jones and Davis' (1965) algorithm.

(c) Perceived correlation.

According to Kelley's (1972b, 1973) ANOVA conception, people organize behavioral information as they receive it into a data matrix whose three dimensions are entities, time, and persons. When called upon to make attributions, they base them upon the stored covariation information. Any inaccuracies in perceived covariation would, of course, lead to erroneous attributions. Just such a discrepancy was found by Chapman and Chapman (1967, 1969; also Golding & Rorer, 1972) who showed that clinical psychologists and clinically naive undergraduates perceive correlations between patients' symptoms and their responses to diagnostic tests which are purely illusory. Kelley (1973, p. 119) attributes these misperceptions to conflict between covariation information and causal preconceptions (schemata). In slightly later work, Tversky and Kahneman (1973) showed that such illusory correlations may be due to the differential memorability or availability of

various symptom-response pairs. Smedslund (1963) who had nurses judge disease-symptom correlations, and Ward and Jenkins (1965; also Jenkins & Ward, 1965) using more artificial tasks, found that when looking at 2 x 2 co-occurrence tables (e.g., [disease, no disease] x [symptom, no symptom]) people base their perceptions of causality solely on the number of cases in which both the disease and the symptom are present. Miller and Ross (1975, pp. 223-4) have capitalized on some of these results to provide a non-motivational explanation of findings that have been interpreted as reflecting a bias toward distorting information to facilitate making self-serving attributions.

(d) Perceptions of randomness.

Before attributing a cause to an event, an observer must decide whether it was caused at all, or whether in the light of the information at his or her disposal, it should be treated as a random event. Certainly causal attributions for random events are worthless. There is a good deal of judgmental evidence showing that people have a very poor conception of randomness. In particular, they don't recognize it when they see it, and offer deterministic explanations of random phenomena (e.g., Kahneman & Tversky, 1972, pp. 434-7). Gamblers' fallacy in the interpretation of random binary series is one well-known example (e.g., Jarvik, 1951; Tune, 1964). Less well known is the corollary of the law of small numbers by which people rarely attribute deviations of results from expectations to sampling variance, because they are always able to find causal explanations for discrepancies (Tversky & Kahneman, 1971). Another is the doggedness and often destructiveness with which people provide causal explanations for regression toward the mean phenomena (Kahneman & Tversky, 1973). An example involving high stakes is Londoners' causal explanations for the pattern of German bombing during World War II, explanations which frequently guided their decisions about where to live and when to seek shelter. Upon later examination the clustering of bomb hits was found to closely approximate a Poisson (random) distribution

(Feller, 1968, p. 160). Burton and Kates (1964) and Kates (1962) provide further costly examples in people's responses to natural hazards.

(e) Cognitive control.

Making proper attributions requires some fairly sophisticated and complicated use of the knowledge accumulated in covariation matrices and causal schemata. There is a good deal of evidence showing that people are poorly equipped for this sort of conditional, multivariate thinking. Hammond and Summers (1972) show that cognitive control, or ability to apply knowledge, may lag well behind the acquisition of that knowledge. They also argue that every-day learning experiences are typically not structured to develop cognitive control.

Much of the problem appears to be related to people's poor insights into both the information integration policies that they are following and the implications of the policies that they would like to be following. Goldberg (1968, 1970) and others have found that in situations in which clinical judges believe that they are performing complicated multivariate judgments, their information-processing policies can be effectively captured by simple linear models, utilizing a relatively small number of variables (e.g., Hoffman, Slovic, & Rorer, 1968). Slovic, Fleissner, and Bauman (1972), studying the judgmental policies of stockbrokers, found a substantial negative correlation between years of experience as a broker and accuracy of self-insight.

One common type of error in many studies (see Slovic & Lichtenstein, 1971, pp. 683-4) is a tendency for judges to overestimate the importance they place on minor cues and underestimate their reliance on a few major variables. All of these results indicate that when introspecting about their own judgmental processes, people tend to exaggerate their information processing sophistication (see also Michael, 1968; Shepard, 1964).

(f) Field Studies.

Although an adequate review is beyond the scope of this paper, sociologists have also identified a number of rich attributional phenomena, primarily pathologies of explanation. Garfinkel's (e.g., 1964, 1966) ethnomethodological works is one source; labeling theory (e.g., Prus, 1975; Schur, 1971) a second; the cataloguing of accounts (Scott & Lyman, 1968) and techniques of neutralization (Rogers & Buffalo, 1974; Sykes & Matza, 1967) a third; and observational studies of gambling behavior (Oldman, 1974; Scott, 1968) a fourth.

Further evidence for the differences between the way social scientists and laypeople think can be found in O'Leary, Coplin, Shapiro and Dean's (1974) study of the explanatory protocols used by U. S. Department of State foreign affairs analysts. They found that whereas academic international relations researchers tended to use small numbers of continuous variables interlinked by simultaneous linear relationships, applied analysts relied on multivariate, explanatory models using discrete variables with non-linear, time-lagged relationships between them. Interestingly, "The kinds of relationships found in the majority of (State Department) analyses represent such complexity that no single quantitative work in the social sciences could even begin to test their validity (p. 228)."

3. Paradigmatic Assumptions

Hopefully, the previous section has shown the natural interface between judgment and attribution research. The present section considers some possible reasons for the preponderance of judgment results suggesting sources of bias in attributional tasks. If attribution and judgment research are seen as tapping the same basic information- processing facility, the divergence in results is better sought in the minds doing the research than the minds being researched.

(a) Probabilistic versus deterministic processes.

The inferential processes hypothesized by both judgment and attribution researchers represent highly deterministic ways of relating to one's environment. The judgment subject is seen as looking for patterns in random sequences, ignoring probabilistic base rate information in favor of individuating information and using relatively few cues from a multitude of potentially valid ones. Probabilistic considerations are almost totally absent in the use of Tversky and Kahneman's heuristics. By like token, the naive attributor seems typically to be viewed as a puzzle-solver who by process of elimination whittles down a set of possible alternative hypotheses.

However, although both fields agree on the basically non-probabilistic nature of people's inference, they disagree on the propriety of that nature. A fundamental notion in judgment research is Brunswik's (1952; also Hammond, 1966) "probabilistic functionalism," the idea that the role of psychology is to study the adaptive interaction between an organism and its uncertain environment. Insofar as that environment is probabilistic, considering the

information at the organism's disposal, deterministic rules of inference are at best approximations. A judgment rule that allows no reflection of probabilistic phenomena known to be operating is by its nature suspect.

Similar suspicion seems lacking in attribution research. Certainly there must be situations in which this is an adequate policy, in which the underlying process generating the behavioral data to be explained has no major probabilistic components, and in which deterministic reasoning will suffice. If the situations studied in attribution research fall into this category, it would not be surprising that attribution subjects perform more adequately than judgment subjects, who are typically confounded by the counter-intuitive nature of the probabilistic processes about which they are called upon to make inferences.

Aside from helping to make people look good, reliance on deterministic tasks (or deterministic aspects of probabilistic tasks) seems to have masked some questions which attribution theorists themselves might find extremely interesting. For example: to what extent are events perceived to be explicable or attributable--in analysis of variance terms, how much of their variance is viewed as explainable? How well do people believe that they have succeeded in explaining events when they have given the best available explanation? If people were asked to estimate the percentage of variance explained by each of several causes, how flat or peaked would their distributions of causal responsibility be in different situations? How many causes would be assigned at least some responsibility? If policy-capturing techniques (Goldberg, 1968; Hammond, Hursch, & Todd, 1968) were applied to sets of attributional judgments, how much insight would people have into their inferred policies? Are there tasks or individuals with predispositions toward unicausal attributions?

Some attribution research, particularly that concerned with perceived causes of success and failure, has elicited attributions to the category of "luck." Presumably, any chance factors impinging upon a success-failure outcome do constitute either good or bad luck--depending upon how things turn out. Yet it is not clear how these results may be generalized to situations in which success-failure is not the primary criterion characterizing outcomes. Nor is it clear whether chance and luck are indeed synonymous even in success-failure situations. It appears, for example, that "luck" is a person-attribution, whereas "chance" is a property of the environment. Nor is it clear how either term relates to those causes or forces which the attributor believes that he could understand with some additonal information, although for the moment they appear inexplicable and random.[3]

(b) Stimulus and response modes.

In the typical judgment study, stimuli are designed to be as complete and unambiguous as possible. The desire to provide subjects with data that cannot be misconstrued and that are sufficient to make the required judgments has, indeed, often resulted in extremely artificial experimental tasks.

Subjects are, however, given little help in knowing how to properly encode these data or relate them to their own previous experience. They are seldom told either what the formal analogue of the experimental task is or which of their previous experiences are at all relevant to handling it. The response mode in most judgment tasks, providing a single subjective probability, gives them no additional hint as to what data are relevant.

Tversky and Kahneman (1974; p. 1130) have claimed that probabilistic inference is so difficult to learn because the proper way to characterize tasks

is often unintuitive or even counterintuitive, and because life experiences are not organized or juxtaposed to reveal their common underlying statistical properties. They write "A person could conceivably learn whether his judgments are externally calibrated by keeping a tally of the proportion of events that actually occur among those to which he assigns the same probability. However, it is not natural to group events by their judged probability. In the absence of such grouping it is impossible for an individual to discover, for example, that only 50 percent of the predictions to which he has assigned a probability of .9 or higher actually came true." If this is the case, then failure to help subjects with encoding may be a source of their downfall in judgment experiments as well as in life.

By contrast, whereas attribution researchers often spend great efforts at guaranteeing the verisimilitude (non-artificiality) of their experimental tasks, critical details of these "slices of life" are typically either presented ambiguously or left entirely unstated. The attribution subject is then asked to infer their values. For example, the observer of a tutorial session might be asked about the tutee, "Was he really trying hard to succeed?" At times, as in some studies of attributional therapy, the stimulus information presented to subjects may be deliberately distorted, leading subjects to reach erroneous attributions by reasoning correctly from false premises (e.g., Storms & Nisbett, 1970).

Attributional subjects are, however, given considerable assistance in structuring this stimulus data. A typical example is MacArthur's (1972) study in which subjects are explicitly told all the dimensions of stimulus characterization relevant to their response. Additional hints of how to look at and organize data can frequently be found in the response format used. For example, asking instructors in a tutoring study to rate the importance of four instructor factors and four student factors in determining their

students' success or failure (Ross, Bierbrauer, & Polly, 1974) must help them know how to structure the data and what the experimenters are looking for. Although the degree of such structuring varies from study to study, it appears to be consistently greater than the degree found in most judgment studies. It must be wondered whether this increase in structure is not enough to get people on the road to reasonable inference. As we noted above, the moderate structuring in Bayesian inference experiments appears to create demand characteristics which usually push people to revise their probabilities in the correct direction even when their intuitive inferential processes are almost unrelated to the proper ones.

Aside from subtly directing subjects' efforts (and possibly improving their performance), the exclusive use of structured, forced-choice response modes seems to have obscured many questions that would be of interest to attribution researchers. How do people organize data for attribution when undirected? Does this subjective organization resemble Kelley's matrices, and if so, what are its dimensions? If asked to fill in such a matrix with data points, could people do it appropriately? If presented with such a matrix in terms of data points rather than empirical generalizations of the type used by MacArthur, would people make the proper generalizations--or would they have difficulties as suggested by the illusory correlation results? If asked to explain an event, when do people naturally produce explanations of "why" rather than of "how" or "what"? When do people feel that events need no explanation at all or that no explanation is adequate? When do people even consider alternative explanations? Does it matter for whom an explanation is being prepared? Frieze (1973, cited in Weiner, 1974, p. 7) is a first step toward answering these questions.

Judgment researchers, too, with a few notable exceptions (Payne, 1975; Kleinmuntz, 1968) have relied on forced choice or probability estimation responses. Elicitation of decision-making protocols or predictive scenarios

would probably reveal hitherto neglected aspects of judgment. To the judgment researchers' credit, though, is the fact that they have experimented systematically with the effects of using different response modes to elicit the probability estimates central to their enterprise (Slovic & Lichtenstein, 1971; p. 698ff). Comparable response mode research has been scarce in attribution work.

(c) Focal issues.

Many inferential tasks have two aspects: (1) evaluating the meaning and relevance of the stimuli presented, and (2) combining the information derived from these stimuli into a judgment. Both of these operations allow for psychological inputs of considerable interest, as the subject decides what's happening and how to put it all together to make sense out of it. Since it is difficult to study two sorts of interrelated psychological phenomena simultaneously, most researchers (with the notable exception of Anderson and his colleagues) have made simplifying assumptions or experimental manipulations regarding either data "valuation" or "integration" (to use Anderson's [1973] terms).

Most judgment research appears to have resolved this dilemma by attempting to eliminate the need for valuation. As suggested in the previous section, stimuli are usually unambiguous and complete, so that the researcher can assume that the subject takes them at face value and adds nothing to them. In addition to the Bayesian inference experiments, excellent examples of this strategy are the "lens model studies" of subjective judgment (e.g., Hammond, Hursch, & Todd, 1964) which define the relevant dimensions of the stimulus object and express its value on each dimension by a simple number. Most experiments are designed to guarantee that subjects have no previous relevant experiences to bring to the task, and have no emotional involvement (other than desire to optimize) which might lead them to distort the data.

As noted by Anderson (1973), much attribution work appears concentrated on questions of valuation. Jones and Nisbett (1971), for example, have shown how differences in perspective lead to differences in the way in which observers and actors valuate behavior. Lerner's work (e.g., 1970) on the attributional consequences of people's "belief in a just world" has shown how generally accepted beliefs can distort people's valuation of data used in their attributions. Research into the attribution of causes for success and failure may be seen as studies of subjects' naive laws about how the world works--do they believe that to succeed on hard tasks one must invest both ability and effort? In order to make confident inferences about how people valuate events from the ways in which they respond to them, the researcher must assume rather simple, straightforward and manageable integration processes. Such an assumption is reasonable when the integration called for is quite simple, either because of the nature of the problem or the help extended to the subject by the experimenter. Most attribution studies fit this requirement. By tapping subjects' previous experiences, attribution studies may afford their subjects an additional advantage. In life, even when people do not possess the programs necessary to solve inferential problems, they may still arrive at a proper solution through trial and error. Judgment research has been directed specifically at helping people solve urgent problems, such as dealing with nuclear power, for which we lack both the proper cognitive programs and the time and resources to learn by trial and error (Slovic, Fischhoff, and Lichtenstein, 1975).

Simplification of either the valuation or the integration operation is, of course, a valid research strategy. The price it carries is some loss of generality. The generalization of current attribution

theories to complex events with probabilistic underlying processes is not obvious. Nor is it clear how existing theories of judgment can incorporate phenomena like wishful thinking or the effects of time and social pressure.

Although a complete understanding of how specific individual decisions are made is impossible without relating to these issues, it is worth noting what judgment researchers hope to gain by their strategy. The goal is an understanding of how well people perform judgmental tasks under the best of conditions. Performance there establishes an asymptotic level beneath which people will perform when confronted with motivational pressures, misleading information, and the like. Establishing this asymptote appears to precede logically encumbering people with these factors.

(d) Use of normative theories.

Obviously, identifying biased behavior requires a clear conception of behavioral adequacy. Generally speaking, such a conception is central to judgment research, tangential to attribution work. It is, then, little wonder that one area has discovered biases and the other has not.

In part, this difference is due to the basic theoretical or philosophical orientation of the two fields. Attribution theory is fundametally phenomenological, attempting to understand what people do when they do what seems right to them. In one context, Kelley (1972a, p. 18) remains unconvinced of the apparent irrationality of a number of seeming "attribution biases;" in another context (1972a, p. 2), he suggests that even when time pressure prevents a complete covariance analysis, "the lay attributor (proceeds) in a reasonable and unbiased manner." Although his covariation principle, which people are believed to follow intuitively, is derived from

a normative theory of behavior, Mills' "Law of Difference," the optimality of subjects' adherence to it is nowhere seriously questioned.

Judgment research has been concerned from the start with the question of subjective optimality; how well people are able to maximize their attainment of subjective criteria. Researchers view cognitive limitations as a major obstacle in such attainment. Moreover, they see suboptimality as a problem with serious social and personal consequences.

> "The regulation of risk poses serious dilemmas for society. policy makers are being asked, with increasing frequency, to "weigh the benefits agains the risks" when making decisions about social and technological programs. These individuals often have highly sophisticated methods at their disposal for gathering information about problems or constructing technological solutions. When it comes to making decisions, however, they typically fall back upon the technique which has been relied upon since antiquity--intuition. The quality of their intuitions sets an upper limit on the quality of the entire decision-making process and, perhaps, the quality of our lives. There is an urgent need to link the study of man's judgmental and decision-making capabilities to the making of decisions that affect the health and safety of the public." (Slovic, Fischhoff, & Lichtenstein, 1976).

In addition, they have sought to improve decision making in areas as diverse as admission to graduate school (Dawes, 1971), investigative radiology (Slovic, Rorer & Hoffman, 1971), experimental design (Tversky & Kahneman, 1971), and adjustment to natural hazards (Slovic, Kunreuther & White, 1974).

Ironically, it would appear as though the more humanistic, phenomenological approach (attribution) shows greater respect for people's intuitive capacities at the price of being able to do relatively little to help them. The ostensibly less humanistic, more mathematically inclined field of judgment may produce useful tools for helping people manage their decisions and learn from experience.

Recent judgment work by Tversky and Kahneman (1974) and others has actually gone in a more phenomenological direction, attempting to understand in non-mechanistic terms, the judgmental heuristics, or rules of thumb, that people use in trying to interpret their world and predict the future. Their focus, however, has been on seeing how these phenomenological rules may lead people astray. A similar compromise would seem possible in the attribution context.

To some extent, however, the relative predominance of normative theories in judgment research seems to reflect extra-psychological realities. Perhaps the foremost of these is the differential accessibility of normative theories for predictive and attributional behavior.

The judgment researcher interested in exploring the descriptive validity of a normative theory of decision making or probabilistic reasoning has no difficulty in finding and learning well developed, easily operationalized and widely agreed upon examples. The subjective expected utility model (Edwards & Tversky, 1968; Feather, 1959; Raiffa, 1968) and the familiar postulates of the probability calculus are but two examples.

By way of contrast, there is no generally agreed upon criterion for explanatory adequacy. There is, instead, a continuing discussion among philosophers of science about what constitutes the proper normative theory. In this debate even the intuitively obvious criterion that a good explanation is one that increases predictive ability is not without its critics. Furthermore, the debate itself is buried in philosophical literature which few psychologists are either familiar with or trained to interpret. Nor have many serious attempts been made to bring to the general public useful statements of the current state of the art, showing how it currently appears that one might best go about making attributions.

In view of this confused state of affairs, it may be tempting to ignore the work of philosophers, at least until they get their normative standards in order. This would, however, be a mistake, for even without producing the "ultimate truth" about explanation, the philosophers have identified logical subtasks that are common to many modes of explanation and attribution and for which behavioral adequacy can be readily defined. If people have difficulty performing any of these subtasks, their ability to meet the demands of any plausible normative theory is suspect. For example, attribution of almost any sort requires simple syllogistic reasoning. testing behavioral laws, and recognizing counter-examples. Wason and Johnson-Laird (1972) have extensively studied the vagaries of subjective inference on tasks like these, results that suggest further problems in naive attribution.

Philosophers can also contribute insight into how to characterize inferential tasks properly. Often there are subtle distinctions between types of tasks that might be casually passed over in planning experiments or developing theories but that may have the greatest consequences in the modes of inference they elicit. Shope (1967), for example, has analyzed in depth the conditions in which it makes sense to speak of the (one) cause of an event rather than the causes, a distinction of obvious importance to attribution work.

The incredible confusion in the attribution of responsibility literature (see Fishbein & Ajzen, 1973; Vidmar & Crinklaw, 1974) due to failure to define the term "responsibility" both precisely and consistently might serve as an illustrative example of how psychologists' vagueness about their basic concepts can strip their work of its value (see also Rozeboom, 1972, 1974).

Since conceptual analysis calls for training that many psychologists lack it seems appropriate to exploit the groundwork already laid by others. Without such broader conceptualizations it may be difficult for attribution research ever to go beyond the substantive areas in which it has made great headway to date and evolve into a general psychology of explanation.

Among the many other topics that should interest attribution theorists and that have received detailed philosophical analysis are: How may causes be weighted (Martin, 1972)? How is an 'event,' the object of an explanation, defined (Pachter, 1974)? When and how may motives be inputed to someone (MacIver, 1940; Paulson, 1972)? How should dispositional attributions be interpreted (Rozeboom, 1973)? What is the logical form of causal statements (Davidson, 1967)? Insightful comments on these questions, and many others, may be found in Hempel (1965). A well-developed multivariate normative model of causal attribution that is quantitative and probabilistic and which has a considerable following is path analysis (e.g., Alwin & Tessler, 1974; Blalock, 1964; Lewis-Beck, 1974). It certainly seems possible to study how good people are as intuitive path analysts. Some general thoughts on the use of "normative models in the study of cognition" may be found in Barclay, Beach, and Braithwaite (1971) and Little (1972).

4. Conclusion

If the above characterization is generally correct, probably the most intriguing question to emerge from it is "Just how good are people as intuitive information processors?" The answer, like the answer to most intriguing questions, appears to be "It depends." I have tried to give some idea of what it depends on, particularly discussing those features of attributional research that appear to encourage or highlight good

performance and those features of judgmental research that appear to discourage or obscure it. A more definitive answer requires both the collection of more directly comparable results, and some conceptual reconciliation between the areas. Whereas I believe that such integration is possible, and have attempted to stress the basic commensurability of these two paradigms, some thorny--and not uninteresting--issues remain to be resolved.

(a) The Role of Error Analysis.

One underlying question is whether one learns more about behavior by asking "What do people do?" or "What do they do wrong?" Attribution and judgment researchers seem to end up asking the first and second questions, respectivley. Whereas a general answer to this question is beyond the scope of the present paper, it is worth noting that a similar conflict has faced investigators in at least one other area of verbal behavior, the applied linguistics of second language learning. There, the respective approaches are called "contrastive" and "error" analysis. As described by Hammarberg (1974), the error analyst attempts to understand the types, frequency and causes of linguistic errors, as well as the degree of disturbance that they cause and how they can be ameliorated. The perceived advantages of this approach to understanding and improving language use arise from the assumptions that errors (1) are evidence of speakers' basic linguistic strategies; (2) reveal how far speakers have progressed in language acquisition and how stable their performance is at that level; and (3) can be used to instruct speakers about their own inadequacies. For those concerned with demand characteristics which might lead subjects to respond unnaturally in order to impress the experimenter, error analysis has an additional advantage. There appears to be no reason why subjects would deliberately respond

incorrectly unless they really believed that they were right. The primary difficulties with error analysis are that it is often difficult to define unambiguously what is an error and what is not and that to help someone, it is frequently crucial to know what people are capable of doing correctly.

A related analogy is Hexter's (1971; p. 51 passim) comparison between explaining historical events and playing the field in baseball. In both pursuits, most "chances" (calls to explain an event or catch a ball) are quite routine, often having habitual, preprogrammed acceptable responses. "Fielding easy chances calls for a very complex set of motions not vastly different from what it takes to field the hard ones, but no one becomes a big leaguer because he can catch an easy grounder and make the easy throw to first. A big leaguer may even make more errors than an amateur, but that is because he gets within reach of balls that others would not even get near. In deciding who is fit to stay in the big leagues, the question is 'Can he field the hard ones?'"

If the analogy drawn above between the respective positions of error analysis and judgment research is indeed valid, detailed examination of the specific issues in the two areas may well prove illuminating.

(b) Mainstreaming.

To some extent, the actual interrelation of the aspects of inferential behavior revealed by judgment and attribution research will only be understood when work in these two areas is properly coupled with what is going on in the rest of psychology. Although the concept of "bounded rationality" so central to judgment work arose out of develop-

ments in cognitive psychology in the 1950's (e.g., Bruner, Goodnow, and Austin, 1956; Miller, 1956; Simon, 1957), little contact has been made since then even with such closely related fields as the study of non-probabilistic information processing. With attribution research the situation is somewhat better (Kanouse, 1972; Kelley, 1973; and Weiner, 1974) yet the field is still autonomous in many respects.

Just as the phenomena described here cannot be fully understood without consideration of their underlying cognitive mechanisms, some of them should provide stimulating inputs for general work in cognition. The hindsight results (Fischhoff, 1975), for example, indicate one way in which semantic memory is reorganized to accommodate new information. The paucity of results and theories showing exactly how Tversky and Kahneman's (1973) availability mechanism might work suggests a need for further research into the process of constrained associates production.

Even more exciting might be exploration of the developmental implications of this work. For example, no theory of cognitive development appears to relate fully to the notion of judgmental biases and heuristics as presented here. Many conceptualizations of cognitive development are primarily concerned with how children acquire the skills that will make them fully functioning adults. The judgment work suggests that we look at how they acquire the heuristics that lead them to be substantially biased information processors, why neither age nor experience appears to eradicate these biases (Goldberg, 1968), and what we might do to educate people to probabilistic, multivariate thinking (Michael, 1968, 1973).

(c) Prediction and explanation.

One obvious prerequisite to integrating work in judgment and attribution is to understand the formal and psychological relationships between prediction and explanation. Common sense appears to hold that the two are highly interrelated, both because of perceived similarities in the underlying processes and because increased prowess in one is seen as conferring increased prowess in the other. When we manage to explain the past, we feel that we have increased our ability to predict the future. The main perceived difference seems to be that we can adequately explain more things than we can predict because we know more in hindsight than in foresight.

Both our dominant philosophy of science, which holds that scientific prediction and explanation are formally identical (e.g., Hempel, 1965), and those attribution theorists who have related to the question (e.g., Kelley, 1972a; Weiner, et al, 1972, p. 96) appear to subscribe to this view.

Hindsight results (Fischhoff, 1974, 1975; Fischhoff & Beyth, 1975) however, indicate that people process information about the past in a way that systematically reduces its perceived surprisingness. We argued that only when confronted with surprises do we feel any need to change our way of looking at and responding to events, i.e., any need to learn. Thus, the very feeling that we have explained, or made sense out of, an event may be the best guarantee that we are not learning anything from it that will improve our predictive efficacy.

One way of interpreting this result would be in terms of the ego defensive bias noted by Kelley (1972a) which reflects our need to feel that the world is controllable. Such a need is certainly served by

exaggerating how much we know about it. Here, as with other forms of denial, the long-range acquisition of coping skills is sacrificed for the short-range illusion of coping ability.

Yet before "resorting" to such a motivational explanation, it is worth considering whether prediction and explanation are formally part of the same process. One argument to the contrary is offered by Hintikka (1968) who distinguishes between "local and global theorizing." (p. 319ff). These are described, respectively, as "on the one hand, a case in which we are predominantly interested in a particular body of observations e which we want to explain by means of a suitable hypothesis h, and on the other hand, a case in which we have no particular interest in our evidence e but rather want to use it as a stepping stone to some general theory h, which is designed to apply to other matters, too, besides e" (p. 321). In the present context, e may be likened to a reported event and h to the set of data and laws from which that event is to be inferred.

Regarding local theorizing, we want to choose the explanatory hypothesis h such that it is maximally informative concerning the subject matter with which e deals. Since we know the truth of e already, we are not interested in the substantive information that h carries concerning the truth of e. What we want to do is to find h such that the truth of e is not unexpected, given h." (ibid). Hintikka then shows that this leads to the choice of h according to the maximum likelihood principle, "A weapon of explanation rather than of generalization." The extent to which the maximum likelihood principle is also a weapon of generalization seems to depend

upon the regularity of the universe from which e has been drawn and to which h may be applied, i.e., to the extent to which "whatever observations we make concerning a part of it can be carried over intact so as to apply to others." (pp. 322-3).

With the unique events considered in most explanations, this irregularity is likely to be substantial. Often our explanations will be so good in the specific case that generalizability is sacrificed. An analogous case can be seen in the regression equation which is "overfit" to a set of data (e.g., by inclusion of too many predictor variables). The price paid for closeness of fit is loss of predictive validity--shrinkage (see also Stover, 1967, p. 54).

Additional biases in explanation will doubtless be forthcoming, particularly in future research that asks open-ended questions like "When do people explain events? For whom do they prepare their explanations? What are their subjective criteria for explanatory adequacy?" In discussing "social psychology's rational man," Abelson (1974) has compiled a partial list of reasons why people may hold beliefs other than for the sake of rationality. Most of these reasons are concered with systems- maintenance, ways of keeping oneself going in a difficult and unpredictable world. They include: as protection against anxiety, as a way to organize vague feelings, and as a means of providing a sense of identity. A similar list of reasons for explaining events other than to increase one's predictive abilities may one day be forthcoming. On that day, we may also be able to help those who are interested in increasing the positive transfer between prediction and explanation.

(d) A possible reconceptualization.

Kelley (1973, p. 112) has likened man to an intuitive scientist. In doing so, he has defined "scientist" by projection, adopting the interpretation commonly accepted among experimental psychologists, that of the intuitive analyzer of variance. Yet, there certainly are other reconstructions of the scientific process (e.g., Kaplan, 1964; Lakatos, 1970) and one might ask whether another conceptualization might be more appropriate.

Probably the most insightful discussions of how and why people do and should explain past events may be found in the ruminations of historians over the state and nature of their craft (e.g., Beard, 1935; Carr, 1961; Commager, 1965; Hexter, 1961; Marwick, 1970; Plumb, 1969). Many of these analyses (e.g., Dray, 1957; Gallie, 1964; Hexter, 1971; Passmore, 1961; Scriven, 1959; Walsh, 1967) have focused on how historical explanation differs in form and purpose from the notion of scientific explanation as proposed in the "covering law model" advanced by Hempel (e.g., 1965) and others. Typically, they argue that historians explain for much the same reasons and in much the same way as ordinary people do. They also discuss the particular training needed to effectively produce explanations of this type. Some of their analyses offer rich inputs to understanding the nature and purpose of "explanation in every-day life, science, and history." (Passmore, 1961).

Before abandoning, as suggested by the judgment results, or embracing, as suggested by attribution theory, the notion of people as intuitive scientists, we should ask what sort of scientists they are or attempt to be. We might get a good deal of mileage out of thinking of ourselves as intuitive historians, and attempting to produce an integrated psychology of predictive and explanatory behavior that accommodates the historians' observations, the philosophers' formalizations and the psychologists' and sociologists' theories and empirical findings.

Footnotes

[1] A related approach, but one which has typically not concerned itself with questions of optimality, is the theory of information integration advanced by Anderson and his associates. Insightful introductions may be found in Anderson (1970, 1971, 1973).

[2] Ajzen and Fishbein's (1975) attempt to conceptualize attribution tasks in Bayesian terms would, of course, provide a common framework for much judgment and attribution work--were it successful. There appear , however, to be some difficulties with their interpretations of both Bayes' Theorem and of the empirical evidence regarding intuitive Bayesian inference, leaving the unifying theory as yet unfound.

[3] A further example of the potentially interesting issues missed by avoidance of probabilistic considerations is Kukla's (1972) attributional theory of performance, which attempts to integrate attribution theory and expectancy theory. After formulating and describing the SEU model of expectancy theory, he eliminates all probabilities for the sake of simplification, leaving a U or expectancy-less model of expectancy. One might also wonder how MacArthur's subjects would have produced attributions from stimuli like "X can fool some of the people some of the time."

[4]It seems appropriate to mention in this context an issue which arose quite early in judgment research, apparently because of the use of quantitative models, and which attribution researchers may find it insightful to work through. It is what Hoffman (1960) called "the paramorphic representation of clinical judgment." Kelley states (1972b, p. 171) that "We do indeed wish and need to know the terms in which the lay attributor thinks about causal problems." Regarding the way to obtain this knowledge he writes (ibid), "It seems unlikely that an 'as if' model that has little correspondence to the attributor's actual modes of information processing will succeed in anticipating and summarizing all the important details of his activities. Hoffman's analysis showed, however, that very different models of information processing may be reflected in identical input-output relationship and may have indistinguishable formal characterizations.

Footnotes

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